

# SCIENCE

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## LABORATORY INSTRUCTION IN PHYSICS.

BY D. W. HERING, UNIVERSITY OF THE CITY OF NEW YORK.

THERE are various practices, and seemingly but two clearly defined methods of teaching physics in college laboratories. The first method, which may be called the progressive one, treats the general subject of physics by going through its various divisions successively, until the whole ground has been covered, whether thoroughly or superficially. For students who have had no preliminary training in physics, this method is the only practicable one if they are to begin their study in the laboratory. The other may be called the method of analysis. It assumes that the pupil has received a fair course of instruction in the principles of the science before he enters upon laboratory work. Then it is a matter of indifference whether his first exercise is one in optics, or in electricity; in radiation, or in specific gravities. He will examine a body of any sort with reference to its various properties, taking account of as many as he can, which in some instances may embrace nearly the whole range of physics. This method then does not present the different features of physics so much as the physical features of different things.

At first sight it would appear as if the method that is pursued for the direct purpose of learning the science would be the one best fitted to give an acquaintance with it, and perhaps this would be true if sufficient time could be given to it to deal with the various branches of physics with tolerable thoroughness, but laboratory work by an untrained pupil is slow at best, and time is limited. It is important, therefore, to follow the plan that will give good results without loss of time.

If physics as a science were distinctly progressive in its nature, one step being essential to a comprehension of the next, and therefore of necessity a preliminary to it, there could be no question as to the best order of proceeding in teaching or in learning the subject. There would still be room for question as to how much should be done by the teacher in experimental illustration with discussions, before putting the pupil to experimenting on his own account, but the order of dealing with the subject in any case would be determined beforehand. But it is thus progressive to only a limited degree. Except for the principles of mechanics, which permeate the entire science, physics, in all its diversity, may be dealt with regardless of the order in which the subjects are taken up. And this exception is not always recognized. Among recent standard text-books which are meant to be especially adapted to laboratory practice, but which mean to omit none of the elementary principles of physics, there is every variety in arrangement of topics. One begins with specific gravity and air pressure, follows with dynamical principles, and presents light as the final subject. Another begins with magnetism, introduces the last third of the work by dynamics, and closes with sound. Still another begins with properties of matter and dynamics and ends with light; while a fourth begins with the mechanical powers and closes with magnetism and electricity. Even the special divisions, as electricity, for example, can scarcely be said to be developed from one principle that necessarily comes first, to another that can be reached only at the end of a well-defined series. Some classification of topics can always be made, but the tendency to-day is to diminish rather than increase the number of classes. Considerations of intrinsic difficulty, or length of time that can be given without interruption, or the season of the year when sunny days may be expected, or other special points may lead to a preference as to the order of subjects, but there is little in the nature of the subjects themselves to determine it.

The status of the student when he is to enter upon the work which this paper is to discuss, will depend upon the manner in which he obtained his first training in physics. He may have acquired his early knowledge by experimentation from the beginning, or he may have been taught from descriptive text-books supplemented by experimental lectures from the teacher, or he may have had a combination of both. In the first case, he had to find out principles and laws as well as (to him) disconnected facts by his own experimentation; in the second, he has been made acquainted with the leading laws and properties and perhaps has had some opportunity to verify and apply them. Whether an attempt to learn physics from the beginning by practice is profitable or advisable has been much discussed, and it is outside of our purpose to enter upon that question. It is a plan that has grown in favor greatly of late, and has been insisted upon by Harvard College, as a preparation for those who are to pursue the subject in college. Let us suppose the pupil to have acquired a general, though elementary, acquaintance with the principles of the science, — that he has reached the standard of at least a well-prepared college junior. For this he has probably been called upon to cover the whole range of the subject whether by experiment, or by recitations and experimental lectures. The advocates of the two methods of preparation will find points to offset one another in the results attained. The experimental student will have acquired his knowledge in a very valuable way, by objective study, by the inductive method. He will have "learned to do by doing." This has become a favorite idea with educators in almost every branch of learning, and its advantages are undeniable in most lines of work, but they are not equally great or equally obvious in all branches or at all stages. It is a most effective way so far as it goes, but in physics the experiments concerning any one point, or involving any law, will have been so small in number under the best opportunities, that the student must infer the law from instances altogether too few and too little varied, to justify an inference. Potent as the inductive method has been in science, its demonstrations are never incontestable, they never rise above a moral certainty, and do not even approach it, if the instances upon which the conclusions are based are not numerous, or else very accurate. The student will in reality have done nothing more than illustrate a point, doing in a crude way what the lecturer before a large class does in a better way. Still the experiment and its results will impress themselves upon him because he did the work himself. In this he will have the advantage of the lecture-taught student. The knowledge of the latter, however, is likely to be more correct as to principles. On the whole, the two classes may be said to approach the higher laboratory practice about equally well equipped: the former better prepared for manipulation with perhaps less readiness to appreciate the science; the latter better prepared to discriminate as to principles, but less expert in determining them. Didactic and experimental instruction are now so well combined in some secondary schools as to make their work superior to that offered in many colleges. Having been fairly well taught by any method, we may suppose the student ready for practical work somewhat more advanced than is to be had in secondary schools, or even in the general course of physics in an average American college. What plan shall be followed in his laboratory work? Presumably that plan is best which is best fitted to accomplish its purpose. What is the purpose of his work? Usually not independent research or original investigation. Work of that class is generally undertaken only by graduates or special students, who are not obliged to accomplish a definite amount in a given time. The higher laboratory work of the college undergraduate is for the purpose of making him practically familiar with physical laws, not in one particular

branch of physics, but throughout the whole subject; for training in making and reducing scientific observations; for acquiring skill in manipulating and adjusting apparatus; all which is to result in giving him a good general knowledge of physics, if he follows the study no further, or to fit him for independent research if such is his design for the future. It is thus intermediate in its thoroughness and definiteness between the preparation in elementary general principles of the science, and the work of the graduate or the advanced special student.

What is likely to be the experience of a student in the college laboratory under what we have called the progressive method, supposing he has time enough in prospect to cover the entire field? He will begin probably with a dozen companions in his division, with the topic placed first in the order chosen, say properties of matter, and dynamics. One of the first operations he will be called upon to perform will be that of weighing a body. The skillful use of a delicate balance, will involve the critical study of the balance itself. This will afford a good exercise in dynamics. To reduce to weight *in vacuo*, will necessitate the reading of the barometer, and an application of the laws of Boyle and of Charles, for the effect of temperature and pressure upon gases. Thus he will have been carried at once beyond the immediate subject of physics to which he was intending to apply himself. However, it was merely an excursion. He may continue with this work until he has learned several modes of weighing, and with several types of apparatus. He must learn to measure time. Here he will be introduced to the use of the pendulum, perhaps the chronograph, and other devices for comparing intervals of time. The method of coincidences will be especially serviceable, if he has a seconds clock and a reversible pendulum, by which to determine the accelerative force of gravity. Atwood's machine, besides illustrating the laws of falling bodies, will serve for critical work in mechanics, if the effect of friction, and the mass of the large pulley are to be considered. Various other exercises in mechanics may be given him; he will hardly go on with less than these, and to each of these he will have given enough time and attention to become proficient in work of this kind, and will have given attention to as little else in physics as possible.

If he passes next to the subject of heat, he will probably remain at this until he has dealt, if possible, as fully with its various phenomena. So far as these phenomena involve mechanics he will have had some especial preparation by his previous work, and now he will be doing that work over again. For instance, in hygrometry the same principles relating to the effect of temperature and pressure upon the volume of a gas, will have to be considered. In specific heats, he will again go over the same kind of work as to masses and densities that he performed with the balance, and so on. But he will do nothing in electricity, even with the heating effect of an electric current, because he has not yet come to electricity. He will do heat pretty thoroughly but not completely. It will be so in each branch he studies. In every one, to do an exercise which is nominally one of a particular class, he must employ principles of classes previously studied. Not until he has gone over the entire range of topics, from the first to the last, will he have taken account of all the principles meant to be included in his course, but when he has done so he will have had a most exhaustive training, for he will then have done nearly everything, not once or twice only, but very many times. His training by that time ought to be excellent, and his knowledge extensive and acute. But to reach such a stage would require longer time or more exclusive devotion to physics than is usually provided for in an undergraduate course. If any thing less is done, however, it means not the omission of certain exercises, but of all the exercises pertaining to one or another entire class of topics. For instance, he may omit sound, or light, wholly. That would make a serious break in his course. By any of the usual arrangements, therefore, the whole subject will be obviously more or less disjointed if it is regarded as made up of members. Fortunately, the highest treatises seek to unify it instead of to dismember it.

How will the student fare by the method of analysis? For an example, give him a piece of plate glass of convenient size. Of

the various determinations regarding this specimen, some will be qualitative, others quantitative. Let him determine:

1. Whether it is regular, and if so, its form. . . . . Qualitative
2. Its dimensions, giving area, thickness and volume. . . . . Quantitative
- 3 Its mass (weighing). . . . . Quantitative
4. If C.G.S. units are employed, this leads at once by dividing mass by volume, to density. . . . . Quantitative
5. But check this by weighing in water, for sp. gr. . . . . Quantitative
6. If the plate is of considerable size, say 25 cms.  $\times$  30 cms. and a small spherometer is available, test the surface for flatness, and map out irregularities. . . . . Quantitative  
(This will serve to show the meaning of instrumental limitations as to precision and accuracy)
7. If possible, compare this with the irregularities of surface, shown by reflection of light, with telescope, or by interference bands when in contact with true "flat." . . . . Qualitative
8. Determine its index of refraction. . . . . Quantitative
9. Its hardness. . . . . Qualitative
10. Its color, by absorption in spectrum. . . . . Qualitative
11. Whether it is homogeneous, by transmission of polarized light. . . . . Qualitative
12. Its specific heat. . . . . Quantitative

Some of these may be out of the reach of many laboratory equipments or only determinable with the help of instruments too delicate to be put into the hands of any but the best students, but still other determinations might be made. Undoubtedly by the time the student has finished such an analysis he will have a very complete knowledge of the specimen he has been working upon, and although, in the instance here cited, the object under scrutiny may seem a trivial one, and the knowledge of its properties no useful addition to his stock of information, not every one may be so. Yet what a range of physics was involved even in this apparently useless analysis! In scientific training it will not have been useless.

As another example, suppose a steel rod be given him to examine. Cutting off a piece about 10 cms. in length, he might determine its dimensions, mass, density, and specific heat. With the long portion he can ascertain its rigidity (by torsion), Young's modulus (by flexure), velocity of sound in it (by longitudinal vibrations), and compare this with the velocity determined from the ratio of elasticity to density. He might magnetize a short piece, say 20 cms., by permanent magnets, and by timing its oscillations, and observing the deflection it gives a needle, determine its magnetic moment, strength of pole, and strength of field in which it swung. Let him then demagnetize it by heating, remagnetize by electric current, and compare its moment now with what it was before. These latter, though not properties of the steel itself, are obtained as consequences of its magnetic character. He might also employ such a magnetized bar to determine its moment of inertia experimentally, and check by calculation from its mass and dimensions. Thus he will have brought into application numerous principles of mechanics, of acoustics, of heat and of magnetism, each of which gives opportunity for work of any required degree of care and precision, involving all the fundamental operations of weighing, measuring, and timing. By the former method he will learn to what extent any one quality is found in numerous specimens examined; by the latter, to what extent these numerous and varied qualities are found in the one specimen examined; by the former he learns one feature of many things, by the latter, many features of one thing. But in learning the one feature he confines his attention chiefly to a few principles of science and needs extend his knowledge of physics no further than to apply these few principles, no matter to how many objects, and there is always the danger of breaking off his work with only a partial view of the science; whereas in learning many features, though confined to only one object, each feature involves one or more distinct principles of science, and the many of them represent a wide range of scientific knowledge. This gives to the student, therefore, indeed it imposes upon him,

a broad culture none the less deep because of its breadth, even if he has had time for the analysis of but one specimen, while the other almost inevitably results in confining his labors and his attainments within narrow limits. Whenever it can be done, a determination of any sort should be made by two processes as nearly independent of each other as possible. For example, the radius of curvature of a lens might be determined by comparing the size of an object, as a scale, with that of its image formed by reflection from the lens surface; and it might be calculated from spherometer measurements. While there are some points in physics which the progressive method would reach and the method of analysis miss, the latter would the more readily lend itself to such twofold determinations.

There are operations such as the calibration of a thermometer, determining a rate of vibration, adjusting special forms of apparatus and determining their constants, etc., that cannot be classified in any simple manner. An attempt to adhere strictly to any clearly defined method throughout the whole course of physics would be unwise and unprofitable. The recognition of a method and of its legitimate limits, however, cannot fail to be of service to a judicious instructor. The limitations of the laboratories themselves in many cases compel a departure from any method and cause the work to degenerate into an unsystematic performing of experiments. It must be admitted, too, that such is the character of the work in some instances where the equipment is very complete.

#### A NEW METHOD OF CHILD STUDY.

BY J. MARK BALDWIN, TORONTO, CANADA.

THE current discussions of the more elementary mental processes show that we lack clearness in our conceptions of the earlier stages of mental life. This is evident enough to call out frequent appeals for "scientific" child study. The word "scientific" is all right, as far as it goes; but as soon as we come to ask what constitutes scientific child study, and why it is that we have so little of it, we find no clear answer, and we go on as before accepting the same anecdotes of fond mothers and repeating the inane observations of Egger and Max Müller.

Of course there are only two ways of studying a child, as of studying any other object—observation and experiment. But who can observe, and who can experiment? Who can look through a telescope and "observe" a new satellite? Only a skillful astronomer. Who can hear a patient's hesitating speech and "observe" aphasia? Only a neurologist. Observation means the acutest exercise of the discriminating faculty of the scientific specialist. And yet most of the observations which we have in this field were made by girls who, before their marriage, knew less about the human body than they did about the moon or a wild flower (having got this latter information from Steele's "Thirteen Weeks") or by a father who sees his child when the boy is dressed up, for an hour a day, and who has never slept in the same room with him in his life; by people who never heard the distinction between reflex and voluntary action, or that between nervous adaptation and conscious selection. Only the psychologist can "observe" the child, and he must be so saturated with his information and his theories that the conduct of the child becomes instinct with meaning for mind and body.

And as for "experiment," greater still is the need. Many a thing a child is said to do—a little judicious experimenting—a little arrangement of the essential requirements of the act in question—shows it is altogether incapable of doing. But to do this we must have our theories, and have our critical moulds arranged beforehand. That most vicious and Philistine attempt in some quarters to put science in the straight-jacket of barren observation, to shut out the life-blood of all science—speculative advance into the secrets of things—this ultra positivistic cry has come here as everywhere else, and put a ban upon theory. On the contrary give us theories, theories, always theories! Let every man who has a theory pronounce his theory! This is just the difference between the mother and the psychologist—she has no theories, he has. She may bring up a family of a dozen and

not be able to make a single thrustworthy observation: he may be able from one sound of one yearling to confirm theories of the neurologist and educator, which are momentous for the future training and welfare of the child.

In the matter of experimenting with children, therefore, our theories must guide our work—guide it into channels which are safe for the growth of the child, stimulating to his powers, definite and enlightening in the outcome. All this has been largely lacking, I think, so far, both in scientific psychology and in applied pedagogy. The implication of physiological and mental is so close in infancy, the mere animal can do so much to ape reason, and the rational is so helpless under the leading of instinct, impulse, and external necessity, that the task is excessively difficult—to say nothing of the extreme delicacy and tenderness of the budding tendrils of the mind. Experiment? Every time we send a child out of the home to the school, we subject him to experiment of the most serious and alarming kind. He goes into the hands of a teacher who is not only not wise unto the child's salvation, but who is on the contrary a machine for administering a single experiment, to an infinite variety of children. It is perfectly certain that two in every three children are irretrievably damaged in their mental and moral development in the school; but I am not at all sure that they would fare any better if they stayed at home! The children are experimented with so much and so unwisely, anyhow, that it is possible that a little experiment, intentionally guided by real insight and psychological information, would do them good.

With this preamble, I wish to call attention to a possible method of experimenting with young children, which has not been before noted to my knowledge. In endeavoring to bring questions like the degree of memory, recognition, association, etc., present in an infant to a practical test, considerable embarrassment has always been experienced in construing safely the child's responses. Of course the only way a child's mind can be studied is through its expression—facial, lingual, vocal, muscular; and the first question, i.e., What did the infant do? must be followed by a second, i.e., What did his doing that mean? And the second question is, as I have said, the harder question, and the one which requires more knowledge and insight. It is evident, on the surface, that the farther away we get in the child's life from simple inherited or reflex responses, the more complicated do the responsive processes become, and the greater becomes the difficulty of analysing them, and arriving at a true picture of the real mental condition which lies back of them.

To illustrate this confusion, I may cite about the one problem which psychologists have attempted to solve by experiments on children, i.e., the determination of the order of rise of the child's perception of the different spectral colors. Preyer starts the series of experiments by showing a child various colors and requiring the child to name them, the results being expressed in percentages of true answers to the whole number. Now this experiment involves no less than four different questions, and the results give absolutely no clue to their analysis. It involves, 1, the child's distinguishing different colors simultaneously displayed before it (i.e., the complete development of the child's color sensation apparatus); 2, the child's ability to recognize or identify a color after having seen it once; 3, an association between the child's color-seeing and word-hearing memories, by which the name is brought up; 4, equally ready facility in the pronunciation of the various color names which the child recognizes: and there is the further embarrassment, that any such process which involves association, is as varied as the lives of children. The single fact that speech is acquired long after objects and some colors are distinguished, shows that Preyer's results are worthless as far as the problem of color perception is concerned.

That the fourth element pointed out above is a real source of confusion is shown by the fact that children recognize many words which they cannot pronounce readily. Binet, who represents the second phase in the development of this experimental problem, realized this, and varied the conditions by naming a color and then requiring the child to pick out the corresponding color. This gave results different not only from Preyer's, but also

from those which Binet reached by Preyer's method. For example, Preyer's child identified yellow better than any other color, a result which no one has confirmed.

The further objection that colors might be distinguished before the word association is established at all, is also seen by Binet, and his attempt to eliminate that source of error constitutes what we may call the third stage in the statement of the problem. He adopts the *méthode de reconnaissance* as preferable to the *méthode d'appellation*. This consisted, in his experiments, in showing to a child a colored counter, and then asking the child to pick out the same color from a number of different colored counters.

This reduces the question to the second of the four I have named above. It is the usual method of testing for color-blindness. It answers very well for color-blindness; for what we really want to learn in the case of a sailor or a signal-man is whether he can recognize a determined color when it is repeated; that is, does he know green or red to be the same as his former experience of green or red. But it is evident that there is still a more fundamental question in the matter — the real question of color perception. It is quite possible a child might not recognize an isolated color quality when he could really very well distinguish color qualities side by side. It is the question just now coming to the front, the question of absolute *vs.* relative recognition, or immediate *vs.* mediate recognition. The last question is this: When does the child get the different color sensations (not recognitions) and in what order?

A further point of criticism of Binet's results serves to illustrate my argument. Binet rules out the influence of the word memories which were necessary to Preyer's results by his *méthode de reconnaissance*. The child recognizes again the color just seen. Now any one who has followed the course of recent discussions of recognition must know that the mediation of word associations is not ruled out in these cases in children of 3 to 5 years old or even younger. Lehmann finds colored wools are recognized when the colors are those whose names are known (*Benennung's association*), and that shades which have not peculiar names, or whose names are not known, are not recognized. Scripture has shown that an unobserved or unintelligible element — a *Nebenvorstellung* — may serve as the link of recognition without rising again to clear consciousness a second time. It is, of course, useless, if these results be trustworthy, to attempt to get recognitions clear of word memories after color names have once been learned by the child. It would seem that the question ought to be taken up with younger children. Binet's experiments were in the interval between the child's 32d and 40th weeks. It is perhaps a confirmation of Lehmann's position, that the colors least recognized in Binet's list are shades whose names are less familiar to children; his list, in order of certainty of recognition, is red, blue, green, rose, maroon, violet, and yellow by the *méthode d'appellation*; and, by both methods together, red, blue, orange, maroon, rose, violet, green, white, and yellow.<sup>1</sup>

This color question may suffice to make clear the essentials of a true experimental method. Only when we catch the motor response in its simplicity is it a true index of the sensory stimulus in its simplicity. I have accordingly attempted to reach a method of child study which would yield a series of experiments whose results would be in terms of the most fundamental motor reactions of the infant, which could be easily and pleasantly conducted, and which would be of wide application. The child's hand-movements are, I think, the most nearly ideal in this respect. The hand reflects the first stimulations, the most stimulations, and, becoming the most mobile and executive organ of volition, attains the most varied and interesting offices of utility. We have spontaneous arm and hand movements, reflex movements, reaching-out movements, grasping movements, imitating movements, manipulating movements, and voluntary efforts — all these, in order, reflecting the development of the mind. The organs of speech are only later brought into use, and their use for speech involves an already high development of mind, hence the error in Preyer's results. It has accordingly seemed to me worth while to find whether a child's reaching movements would reflect

with any degree of regularity the modifications of its sensibility, and, if so, how far this could be made a method of experimenting with young children.

Before speaking, however, of applications, I may adduce one or two other considerations which tend to show that some such dynamogenic method is theoretically valid. Féré showed that sensory stimulations of all kinds increase the maximum hand-pressure. Colors (seen) have regular and each its peculiar effect upon movement. Tones have similar influence. The ticking of a watch is more clearly perceived if a sound is heard at the same time. Further, the reaction-time of hand-movements is shorter if the stimulus (sound, etc.) be more intense. There is an enlargement of the hand, through increased blood-pressure, when a loud sound is heard. These, and a variety of other facts upon which the law of dynamogenesis rests, seem to afford justification for the view that the infant's hand-movements (say) in reaching and grasping will be an index of the kind and intensity of its sensory experiences. Magendie<sup>2</sup> long ago suggested measuring changes in sensibility by the corresponding changes in blood-pressure.

Further, it is not necessary to embarrass ourselves with the question whether the hand-movements are voluntary or not. However we may differ as to the circumstances of the rise of volition, it is still true that after its rise the child's reactions are for a long time quite under the lead of its sensory life. It lives so fully in the immediate present and so closely in touch with its environment, that the influences which lead to movement can be detected with great regularity. In this case the sensations, which are movement-stimuli, become what we may call "effort-stimuli," and the child's hand-efforts become our indications of the relative degree of discrimination, attractiveness, etc., of the different sensations.

Suppose we hang a piece of meat up over Carlo's head and tell him to jump for it. His first jump falls short of the meat. He jumps again and clears a greater distance. Why does he jump farther the second time? Not because he argues that a harder jump is necessary to secure the meat, but because by the first jump he got more smell, blood-color, and appetite-stimulus from the meat. Now suppose it be a red rag instead of meat, and Carlo refuse to jump a second time. This is not because he concludes the rag would choke him, but because he gets a kind of sensation which takes away what appetite-stimulus he already had. The thing is a thing of sensational dynamogeny or "suggestion," and the child-state up to his 24th month (more or less) is just about the same.

The following questions, I think, might be taken up by this method:—

1. The presence of different color-sensations as shown by the number and persistence of the child's effort to grasp the color.
2. The relative attractiveness of different colors measured in the same way.
3. The relative attractiveness of different color combinations.
4. The relative exactness of distance-estimation as shown by the child's efforts to reach over distances for objects.
5. The relative attractiveness of different visual outlines (stars, circles, etc.) cut in the same attractive color, etc.
6. The relative use of right, left, and both hands.
7. The rise of imitative movements.
8. The rise of voluntary movements.

I am quite aware of the meagreness of this list; but one has only to remember the fact that there is no such thing yet as a psycho-physics of the active life, that this side of psychology is *terra incognita* to the experimentalist.<sup>3</sup> If the method proves reliable in one-half of these questions, then so much gain. I have applied it to questions 1, 2, 4, 6, and 7 with results, some of which I have already published in this journal. Other papers will be devoted to these detailed applications.

<sup>2</sup> Féré, "Sensation et Movement," p. 56.

<sup>3</sup> I see no reason that a method could not be devised for testing the motive influences of presentations in terms of the time elapsed since their experience. I have announced elsewhere (*Proceedings of Congress for Exper. Psychology*, London, 1892), the first results of a research conducted upon adults by such a method.

<sup>1</sup> Calculated from Binet's detailed results (*Revue Philosophique*, 1890, II., 582 ff.) by Mr. F. Tracy.

## JAPANESE NURSERY NOTES.

BY ALBERT S. ASHMEAD, M.D., NEW YORK CITY.

It has often been said that the Japanese are the most interesting, the strangest, even the quaintest, people we know. In no regard is this truer than in the care they take of their babies. Such a strong foundation is more necessary than elsewhere, to a nation where man is born to remain a baby his whole life. We, destined to exercise stronger and more serious minds, would be, at the very beginning of our existence, deteriorated by such ingenuous, untiring care.

I have spoken in another article<sup>1</sup> of the long-continued lactation of Japanese women, as benefiting both mother and child; also of the care taken of pregnant women, in which a solicitude displays itself, at the same time, clever and loving. This tender and intelligent attention paid to the born baby is the second part of that unique Japanese system of rearing healthy and happy men, which makes European and American ladies forget that so many other Japanese conceits are a severe shock to their feelings. During the dentition period the children have an extra diet, consisting of fish and small crustaceæ. Japanese not being a carnivorous people, this is natural enough. If they ate meat, they would give their children beef very likely. But it is certainly to the advantage of the bony structure of the child to be, on first entering the adult course of eating, fed in the Japanese manner.

The abominable diaper is unknown to the Japanese. They use only a breech-clout, which is removed at the moment of defecation. The child then is put in such a position that its legs straddle the arms, his body and head resting against the abdomen of the parent, who, gently rocking it in a certain rhythmical, tentative fashion, and accompanying his action with a kind of low whistling, reminding you of a lullaby, gives his offspring its first lesson in personal cleanliness, which, to the Japanese mind, is exceedingly next to godliness. It will be seen how, by this method, unnatural positions are avoided, a thing the more important that Japan is the country of worms, distomata, etc.

It is known probably to every reading person that Japan, like all oriental lands, is, for obvious reasons, furnitureless. It does not even know the cradle. As Diogenes made a cup of his hollow hand, thus the Japanese mother makes a cradle with the back of an older child, an ambulating, delightful cradle, where it stays from morning to night, and is unrhythmically rocked according to the chances and sports which the day offers to its patient and loving victim. Her back, of course, is its first cradle; when it wants the breast, it reaches over or under her arm for it.

The cause of the absence of furniture is the presence of tropical vermin. This awful presence is probably also the cause of the carpetless state of the nursery. The floor is covered with stuffed straw mats, thick and elastic; it is the usual floor of a Japanese house. The floor is mopped every day with salt water; it is, in fact, a chlorine wash. It must be remembered that in Japan the dirt of the street is not carried into the room, sandals and shoes being left at the front door. The necessity of keeping the floor in a sanitary condition is more important in Japan than anywhere else, because of the national habit not only of sitting, but sleeping on the floor.

There is a singular difference between the carriage of Japanese children and the way in which our children walk and move about. The Japanese urchin, whose feet never knew the unkind pressure of tight shoes, and, in fact, no pressure at all, walks more erect, is more sure-footed. In fair weather he wears flat straw-sandals; in these sandals the big toe is widely separated from the others, which gives the child a surer foundation. In wet weather he must maintain his equilibrium on his stilt-like wooden clogs, which keep his feet dry, at the same time compelling him to acquire an extraordinary power over his own motions.

There is in Japan no kissing, not even in the nursery. All the dangers, which have been so elegantly described in newspapers some time ago, arising from the touch of lips, in human love

directly, and at the communion table indirectly, are avoided by the national aversion for labial contact.

The sexes are separated at an early age, and the separation is maintained until marriage. After marriage the husband has a right to annex to his household as many concubines as his means allow. If his wife is delicate, she will perhaps suggest some friend of hers, who will prove rather an ally than a rival. At any rate, there will be no diminution of the friendship between the two women. When pregnancy occurs, a second concubine may be suggested, and no such addition ever troubles the quiet waters of a Japanese household. It is incredible of what amount of peace and, consequently, happiness, the absence of the green-monster alone may be the cause.

When she loses a child, the Japanese mother does not wring her hands and look up to heaven; she sits with folded hands, sunken head, her eyes looking into her lap. Japanese grief has been very eloquently described by my colleague in Japan, Professor Wernich, and I think it will be a good winding-up of this little article if I quote a passage of his remarkable book, "Geographico-Medical Studies;" "However often I have witnessed the death of dear relations, children, for instance, or husbands, I never had occasion to observe the wringing of hands, to which European women of the lower classes are so much addicted. A bitter sorrow was expressed through deep sinking of the head, grasping the hands together, shedding of tears. That strong mental agony, which digs into the soul, so to speak, and takes hold of it, like a bodily pain, seems to be unknown to them. They never 'turn to heaven their faces bathed in tears,' an action which to us seems not only natural and in perfect accord with the essence of grief, but is considered as beautiful and as a worthy subject of artistic representation. In prayers the Japanese mother does not lift her eyes to heaven; with bent head, the body somewhat shrunk together, with hands put together by the palms and slightly raised to the level of the chin, she sends her humble prayer, for quite concrete things, you may be sure, to Buddha."

## NOTES AND NEWS.

PROFESSOR MARTIN, on account of his serious and prolonged ill health, has tendered his resignation of the professorship of biology which he has held in the Johns Hopkins University since 1876.

— The third volume of "Hermetic Philosophy," by Styx, has just been issued from the press of J. B. Lippincott Company of Philadelphia, completing the work. It is quite different in form from the other two volumes, being a dialogue in imitation of Plato on the question, "Can virtue and science be taught?" The author says in his preface: "Having already written two volumes on the essential teachings of the Hermetic Philosophy, and finding that they are not profitable attractions, as we hoped they would be, we have concluded to vary the performance." Whether the new volume will prove more attractive than the earlier ones or not may perhaps be doubted; but it is more readable, and contains much less of the peculiar stuff known as occultism than they did, though it contains enough for most readers. A large part of the book is taken up with ridicule of the "Christian scientists," which is suggestive of the dispute between the pot and the kettle; but a good deal of space is also devoted to setting forth the theosophical doctrine of reminiscence and reincarnation, or, in other words, the transmigration of souls. The question about the nature of virtue and whether virtue can be taught is discussed in various aspects; but the light that is shed upon it is rather what Milton calls "darkness visible." The production of several works of this kind in these times seems to us a singular phenomenon, hardly to be accounted for on ordinary principles; but it is apparently due to the general break-up of the old creeds, which has left a vacuum that men seek to fill with some doctrine or other, true, false, or nonsensical, as the case may be. For our part, we cannot conceive how any one can write such books, and, as we read them, we cannot help thinking that the authors do not really believe the doctrines they set forth.

<sup>1</sup> "On the Non-Existence of Rachitis in Japan," Medical Record, Oct. 11, 1890.



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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

EVIDENCE OF TWO PRE-MORAINIC GLACIAL MOVEMENTS.<sup>1</sup>

BY G. D. SWEZEY, DOANE COLLEGE, CRETE, NEBRASKA.

THE valley of Rock River, running southward through southern Wisconsin and northern Illinois, is a very deep pre glacial erosion gorge, cut through Lower Silurian rocks and filled with assorted, incoherent glacial sands and gravels. Artesian wells go down several hundred feet through these gravels before striking rock. The region lies to the south of the great terminal moraine.

Along the bluffs of this valley, at various points, there is exposed a conglomerate, composed also of glacial gravels; it is a pudding stone, thoroughly indurated, so as to make an excellent rock for cellar-walls and the like; in some cases it is cemented together by a calcareous matrix, in other cases there is a large percentage of iron hydroxide in the cementing material.

I have long known some of these outcrops, and have been puzzled by them; but have supposed that they were due to local causes affecting the gravels which everywhere fill the broad valley; but some more careful observations this summer reveal these facts:—

1. They rise, in every case that I have observed, ten to twenty feet above the river bottoms.
2. They are overlaid in some cases by the boulder-clay, containing unassorted, striated pebbles.
3. They occur, so far as I can discover, and I am pretty familiar with the region, which was my native county, only on the extreme edge of the bluffs overlooking the broad river bottoms, or on the bluffs of valleys of some width which were tributaries to the main valley when its latest bottoms were formed. They appear, in other words, to be remnants of older gravels which once filled the valley, but were mostly cut away by the floods which deposited the later, unconsolidated gravels now filling the valley and constituting its flood-plain at a level of ten to twenty feet below the top of the conglomerate. In one place the extreme face of the bluff, immediately below an outcrop of the conglomerate, was made up of layers of the light-colored, incoherent gravels, alternating with dark, iron-stained material, evidently derived from the older conglomerate, which then formed the bluffs against which the stream washed.

This distinction of age is confirmed by the occurrence of the ground moraine of the latest glacial movement in this region, overlying the consolidated gravels.

There is no decisive evidence that the interval between these movements was one of great duration, but the striking contrast in appearance between the loose gravels and the conglomerate tends to impress one with the idea that the latter is relatively very old. So striking is this appearance, that at one exposure which I visited I found the owner of the field laboriously digging up an outcropping mass of this conglomerate, somewhat harder and redder than usual, under the supposition that it was a mete-

orite, which he purposes to take in to Chicago next year for an exhibit.

A further consideration, which would seem to imply a considerable interval between the two movements, is that the following succession of events would seem called for to explain the facts:—

1. A glacial movement bringing the material of which the conglomerate is composed, and which includes about the usual proportion of local and remote ingredients.
2. A melting of the ice and floods, surpassing in extent those of the later epoch, for the conglomerate, as before stated, lies regularly at a higher level than the later gravels.
3. Another forward movement of the ice to account for the ground moraine overlying the conglomerate; and
4. Another melting of the ice to deposit the later gravels.

## SOME NOTES ON LIGHTHOUSE APPARATUS.

BY J. KENWARD, C.E., F.S.A., BIRMINGHAM, ENGLAND.

IN 1851, the United States possessed four sea-lights on the dioptric system. In 1891 the number was of sea-lights 138, of harbor lights 526, in addition to about 100 of the small apparatus called range-lenses and lens-lanterns.

This magnificent progress in forty years reflects the highest credit on the Government and on its nautical and engineering officers. Under official auspices in 1851, a most exhaustive enquiry was promoted into the merits of the dioptric or refracting system of lights of Augustin Fresnel in comparison with the catoptric or reflecting system which it had begun to supersede. The result having been ascertained to prove a sevenfold superiority for the dioptric system, the government authorized the lighting of the United States coast-line on an imposing scale, and it has ever since taken a watchful and intelligent interest in the advancement of lighthouse science, and in the gradual provision of the best forms of optical and mechanical apparatus.

The steps of progress, indeed, in lighthouse design and construction have been many and important. The first home of this industry was in France, where the illustrious mathematician and physicist to whose practical genius the lenticular system is due, lived his short life, dying in 1827. The celebrated Tour de Cordouan, at the mouth of the Garonne, was the first lighthouse to receive the new installation of his lenses. The names of Leonar Fresnel, brother of Augustin, of Soleil, Letourneau, Lepaute, Sautter, Barbier, Degrand, Allard, Reynaud, Bourdelles, Bernard, and others follow in brilliant succession in France, as engineers, constructors, or contributors to the literature of the subject; while in the United Kingdom the great family of the Stevensons, Mr. James Chance, Dr. John Hopkinson, Sir James Douglass and Mr. Wigham of Dublin, may be cited as equally distinguished.

Nor have the authorities of the United States, while availing themselves fully of the labors and researches of all these experts, been backward in adding American names to the list of honor. To mention only three, General Alexander, Major George Elliot, and Major D. P. Heap are worthy, in their special work, of the country of such men of science as Professor Henry and Professor Newcomb.

It is particularly to Major Heap of New York that credit is due not only in selecting the most novel and striking forms of apparatus produced in Europe, but also in promoting the design and construction in the States, of the lanterns, lamps, clockwork, pedestals, etc., which are indispensable to it. Major Heap is the author, too, of an excellent compilation on lighthouses.

Let me glance at some of the past achievements and present resources of lighthouse science.

During the past ten or fifteen years the great extension of commerce, the opening of new ports, the multiplication of steam vessels of all classes, and the striking acceleration of their speed, have affected lighthouses and lightships in the three essential points of number, power, and distinctiveness. The chief maritime countries of the world—the United States, Great Britain and her colonies and dependencies, France, Holland, Italy, and Denmark, have endowed their coasts with an imposing array of

<sup>1</sup> Paper read before the Nebraska Academy of Sciences, Dec. 27, 1892.

lights, and their needs are not nearly satisfied. It was eloquently said by the Secretary of State at the International Marine Conference at Washington in 1889: "The spoken languages of the world will continue to be many, but necessity commands that the unspoken language of the sea shall be one." Thus the signal-lighting of the sea-coasts and of the ships traversing the sea is a work truly and emphatically international—a work which neither in its magnitude nor its variety suggests any notion of finality. For instance, the English Admiralty published in 1862 forty notices to mariners in relation to lights, buoys, dangers, etc., in all the work. In 1892 its similar notices amounted to over 600.

The considerations of power or intensity and of distinctiveness were naturally on the numerical increase of lighthouses. The early illuminating apparatus of Fresnel and his successors were mainly confined to two forms, fixed and revolving, the latter being approximately from six to eight times as powerful as the former, the four-wick vegetable oil flame being the brightest illuminant in both. Subsequently composite lights of both fixed and revolving sections were adopted, as well as more effective flames. Next followed the important enhancement of revolving apparatus by the holophotal system of Thomas Stevenson, who also introduced the use of condensing prisms and of mirrors for fixed lights, and who lastly, after many minor improvements, suggested the maximum size of lens yet attained, called the hyper-radial—a light particularly suited to great headlands and other stations where ranges of visibility of thirty or forty miles are necessary. The increase *pari passu* of the potency of lamps—thanks chiefly to the unwearied and intelligent labors of Sir James Douglass with petroleum, and of Mr. Wigham of Dublin with gas—has given due effect to these great developments of dimension. The first-order revolving light shortly to be established on Heceta Head, Oregon, the work of Messrs. Chance of England, is an example of the holophotal system with twenty six prisms of a radius of 920 millimetres. Mosquito Inlet, Florida, erected in 1887, is an example of a hyper-radial fixed light of 1330 millimetres radius, with prisms, on the holophotal system. This was constructed by Messrs. Barbier & Co. of Paris. Dondra Head and Barbery, Ceylon, are examples of hyper-radial revolving lights without prisms, and with lenses of 80° vertical angle. These lights are the work of Messrs. Chance.

Power or intensity of beam has also been attained by superposing one lens apparatus on another, increasing *pro tanto* the total effectiveness of the light. Mr. Brown of Lewisham, England, was the first (in 1859) to propose this arrangement of lenses, and Mr. Wigham of Dublin the first (1872) to carry it out in some fine Irish lights constructed by French makers. Messrs. Chance have since constructed striking examples of the biform type for Bishop Rock and Round Island, Scilly Isles, for the Bull Rock, Ireland, and for the Eddystone, English Channel.

A further method of intensifying lighthouse beams is the electric illuminant. Here the United States have not been backward in following the example of Great Britain and France, though the American use has been more conspicuous in buoys and beacons than in sea-lights like the St. Catherine's in England, the Isle of May in Scotland, or the Cap Grisnez or Ushant in France.

It is, however, beginning to be understood that the electric light in its present condition is not, save in a few cases, to be too strongly recommended for lighthouse service. Its cost, when applied to large apparatus, both for instalment and for maintenance is very considerable, and in thick weather its superiority of penetration to the rays of gas or oil lamps of the present imposing dimensions is a much controverted point. It is, indeed, a mixed question, for the lighthouse engineer and the financial secretary, to be determined according to the nautical and economical conditions of each station. Yet it must never be forgotten that the true way of estimating the combined effectiveness and expense of a light is to divide the units of first cost and annual maintenance by the units of power or intensity.

A more important consideration seems to be that of distinctiveness. The early French plan of making a portion of the apparatus of fixed optical sections and a portion of revolving optical

sections has the obvious disability of inequality of range where the light is white throughout or red throughout, so that a vessel observing the flash at a certain distance can only see the fixed beam at a much less distance, and can only know the true character of the light after an interval more or less prolonged. When color is used for the revolving portion this difference is no doubt much diminished, but it is still too great. A light wholly of revolving sections is preferable for all purposes in these days of high speed and multiplied traffic. And it is the revolving light that affords a much greater number of characteristics than the fixed. The group-flashing system in double and triple series by optical combinations, first introduced by Dr. John Hopkinson and Messrs. Chance in 1875, has been adopted all over the world; and the gas group-flashing system of Mr. Wigham, of about the same date, has been extensively used in Ireland. Combinations of red and white flashing lights, where the former is rendered as powerful as the latter by special optical contrivances, have been repeatedly employed, especially by Messrs. Chance.

A very valuable form of distinction largely adopted in modern times, is the occulting light, that is, a fixed beam interrupted by a short darkness instead of a long darkness interrupted by a short flash as in a revolving light. The sharp contrast of dark and light is thus substituted for the old fixed light, and this is particularly valuable for ports and harbors where shore and ship lights are now so much more numerous and powerful than before. A very simple form of clockwork, designed by the writer, gives movement to the occulting screens for small lights and to the vertically dropping screen which is preferable for sea lights where the occulting system is accounted powerful enough for a sea-light. In occulting lights care should be taken that the duration of darkness should be sufficient to affect the eye sensibly, as, for instance, two seconds at least. There is a growing tendency, also, to make the flashes of revolving lights too short in duration or too quick in recurrence. The difficulty of identifying a light and of taking a bearing by it is thus much increased, and the wear and tear of the mechanism for rotating the apparatus becomes very serious despite the recent expedient of a mercury trough in which the framework revolves.

The most approved optical and mechanical arrangements for our lighthouses would be of little avail if the aliment oil or gas, which sustains the greater number of them, were of unsuitable quality. As regards gas there need be no other provision than that made for the town or harbor supply near the lighthouse, and the only need is to use it with adequate pressure in an appropriate burner in single or multiple jets or rings. As regards oil, the different vegetable varieties, the chief of which was colza, have now nearly fallen into disuse, giving place to petroleum in some of its many forms. The luminiferous properties of good oil and good gas are almost equal, but the cost of petroleum is not more than one-fourth that of vegetable oil, and not greater than that of gas, while its extreme pliability and convenience make it quite as valuable as gas. The lighthouse world is indebted to an American, Captain Doty, for first showing, twenty years ago, how mineral oil could be used in a multiple-wick burner, and it is indebted also to America for the largest and best supplies of the oil itself. The only drawback has been the undoubted greater risk of fire and explosion, but even this has been obviated by the introduction of the variety called "heavy mineral oil," which, having a flashing point of 240° to 270°, is almost absolutely safe. It is now generally used in Europe.

The improvement of burners fit for mineral illuminants has proceeded, as I have said, well nigh to perfection in the hands of the Trinity House of London and of their late engineer, Sir James Douglass. His six-wick burner is of the power of about 900 candles consuming about  $\frac{9}{16}$  of a gallon per hour; his ten-wick burner is of about 2,200 candles, consuming about  $1\frac{7}{10}$  of a gallon per hour. I believe that no form of the Doty or any other oil-burner equals this. The pressure-lamps of Messrs. Chance used with such burners seem to secure the maximum of advantage in the focus of any dioptric sea-light. Very much excellent work, however, in the way of improvement of burners, has been achieved by Major Heap and by Mr. Funck, his assistant at Tompkinsville.

It is a singular fact that, despite all the improvements of the dioptric system and the vital urgency of the matter, the side and mast lights of vessels still remain to a large extent in so imperfect a condition. In Paris and Birmingham, the only seats of the manufacture of dioptric lights, ship lights with true lenses have long been constructed on the same principles of the sea-lights which have a radius thirteen times as great. The writer has long urged, both publicly and privately, the employment of more powerful lights at sea, and more particularly the equalization of the power of these lights by using electricity in incandescent lamps of unequal intensity, in the colored side lights, so that meeting or passing vessels shall understand the course and character of each other at much greater distances than are now sanctioned by statutory rules. At the International Marine Conference in Washington, in 1889, the subject of ship lights was amply discussed with reference to azimuthal ranges and vertical divergences, and the conclusions formulated are being now internationally adopted. But the question of *greater intensity of beam* and of *equality of beam* does not appear to have been considered in relation to the greatly changed conditions of vessels thronging the high and narrow seas in these days, and to the ever-increasing frequency of accidents by collision at night. I earnestly hope that the authorities of the United States will yet again take the initiative in effecting this final improvement in ship lights.

In closing for the present these few remarks on lighthouses it is impossible not to give expression to feelings of admiration for the liberal and enlightened policy of the United States in maintaining the lighthouses of their immense coast-line free of toll to all the maritime world. America sets a shining example to many an older country in this as in many other ways. May her maritime prosperity abundantly increase!

#### A JAPANESE SICK WITH SCARLET-FEVER.<sup>1</sup>

BY ALBERT S. ASHMEAD, M.D., NEW YORK CITY.

I HAVE been introduced to a Japanese gentleman, aged 23, living in Brooklyn, who is undergoing treatment by Dr. Benjamin Ayres for scarlet-fever. As this is the first case of scarlet-fever I have ever seen in a Japanese, I report it to you. To-day is the twenty-eighth day of the disease. There has been no temperature during the last two weeks. Desquamation has been general for three weeks, mostly behind the knees and about the shoulders. He has now scaly desquamation on the palms and soles; noticed first by the patient on the backs of the hands. The throat showed very marked symptoms and is even now very distinctly red and inflamed. Highest temperature  $103\frac{1}{2}$ ; no albumenuria.

I content myself with this short sketch, as, I think, Dr. Ayres will make a more complete report.

I am the more interested in this case, as it is supposed that the Japanese have an immunity from scarlet-fever. I have tried, without success, several times to inoculate a Japanese subject with the disease, in the hope of producing a protective virus. More recently I inoculated two children who had been exposed to the contagion of scarlet-fever with the blood-serum from a blister on the body of a child who, having had scarlet-fever previously, was artificially immune.

These children, whether protected or not, did not take the disease. More recently still, I have inoculated two cases of scarlet-fever with pure blood-serum from a blister on the body of an adult, who was also artificially immune. The inoculations were made in the arms on the third, fourth, and fifth days. In these latter cases there was no effect if diminished desquamation is not to be considered as one. Both cases ran a mild course. It is my opinion, on which, having so little to go upon, I would not insist too strongly, that blood-serum from an artificially immune subject has a virtue, if not curative, at least preventive. Dr. Seward of the Willard Parker Hospital promised me to make a further investigation in the scarlet-fever ward of his hospital.

I have given you these facts to show you what reasons I have to be particularly interested in the case on which I have summarily reported.

<sup>1</sup> Communicated to the Tei-I-Kwai.

#### ELECTRICAL NOTES.

IF a student of molecular physics had been asked a few months ago for an explanation of the phenomenon seen when an electrical discharge is passed through a Geissler tube, he would not have hesitated in his reply. He would have shown, from the researches of J. J. Thomson and others, that the phenomenon, in the case of the non-striated discharge, is akin to that of electrolysis, that disassociation was a necessary accompaniment; that, in the case of the striated discharge, the electricity was carried partly by convection and partly by electrolysis, that this was shown by the fact that the conduction did not proceed with the velocity of light, that each stria was a place where electrolysis was taking place, and each dark band a place where the electricity was carried by convection, that the reason why the discharge was not produced with mercury vapor is that it cannot be disassociated, and that the reason that it takes place so readily with other gases is that the converse is the case.

But the recent work of Herr Hertz and Dr. Lenard has caused considerable doubt to be thrown on some parts of this theory. Not that the theory as given above may not be true after all, but it must first explain the phenomena discovered by the above-named scientists, and at present this seems difficult.

A short account of them is as follows: If we take a Crookes tube, i. e., a tube in which exhaustion has been carried to such an extent that the discharge is no longer visible, except where it strikes upon the glass, or some other solid or phosphorescent substance, we find that, as the exhaustion progresses, the rays issuing from the cathode, and producing incandescence or phosphorescence, instead of passing directly from the cathode to the anode, tend to move in a straight line, normal to the cathode. This discharge has been supposed, one might almost say proved, by Crookes, in a series of most masterly experiments, to consist of highly charged atoms of gas, repelled with great violence from the cathode. As the exhaustion becomes more and more thorough, fewer and fewer atoms are left in the tube, and consequently the trajectories of the atoms become more and more nearly straight lines, and, if the tube is bent at an angle between the electrodes, the discharge will strike against the glass.

If this is the real nature of the discharge, it would seem on first sight that it should not be able to pass through a metallic substance. Yet it has been discovered by Herr Hertz that this is not the case, that it passes readily through thin metal plates. From these two facts, that the discharge takes place in straight lines, and that it passes through thin metal plates, Dr. Lenard conceived the idea that it should be possible to produce the discharge in a Crookes tube and make it pass out into the air, and the experiment, when tried, proved successful.

The apparatus used was as follows: A Crookes tube, whose two ends we will call A and B, had the cathode electrode sealed in at A. This was of the usual form, and projected some distance into the tube. The anode consisted of a tube of aluminium, only a little smaller than the size of the glass tube containing it, and surrounding the cathode. On the discharge taking place it would, instead of passing directly from the cathode to the anode, as in the case where the gas was not so much rarefied, proceed normally from the cathode and out of the open end of the aluminium tube constituting the anode and strike against the glass at the other end of the Crookes tube. In these experiments, that end was cut off, and a metal plate cemented across the opening. In the middle of this metal plate a small hole, 1.7 millimetres, was drilled, and this was covered by a sheet of aluminium, .0003 millimetres thick. Consequently, when the discharge struck against the aluminium plate, the latter being permeable to it, it passed out into the air. This was shown by a luminous discharge just outside the sheet of aluminium, and by the fact that phosphorescent substances placed there behaved in the same manner as when exposed to the cathode discharge in a Crookes tube. If, in place of air, other gases were made to surround the aluminium plate, very different effects were obtained. If the gas was hydrogen, the discharge, after passing through the aluminium window, was not scattered so much. If carbonic acid gas, the scattering was much greater. Dr. Lenard pointed out that, as all gases at



the same temperature have the same number of molecules to the cubic centimetre, this shows that it is not the number but the kind of molecules which determines the scattering. But perhaps the most important experiments were those in which the discharge was allowed to pass into another tube which had been exhausted so far as possible. It was argued that if the cathode discharge was due to the projection of atoms from the cathode that it could not take place in an absolute vacuum. The tube into which the discharge was to pass was exhausted as far as possible, i.e., until a twenty-centimetre spark would not pass from one electrode of the absolute vacuum tube to the other. Notwithstanding this extreme exhaustion, the discharge passed freely through, as was shown by the phosphorescence of substances placed at the other end. The conclusion which Dr. Lenard draws from this experiment is that the cathode rays are really processes in the ether, and not due to the movement of atoms.

On account of the difficulty of obtaining an absolute vacuum, Dr. Lenard's results cannot be accepted as final. Even at the exhaustion obtained by him it may be calculated that there are quite a sufficient number of atoms left to produce the phenomenon (using the results of J. J. Thomson and Chattock in the calculation), even neglecting the number contained in the layer of air on the sides of the tube, and which would be driven off into the tube so soon as the discharge began to pass. Moreover, it is quite possible to conceive that a discharge of atoms from the cathode, on reaching a thin metal sheet, and being abruptly stopped by it, might propagate an electric disturbance proceeding from the other side of the sheet of metal, and so drive off another set of charged atoms. If there were any way of obtaining an absolute vacuum, of course the question could be answered definitely, but this is impossible, and we must wait for further results before attempting an explanation.

R. A. F.

#### LETTERS TO THE EDITOR.

*\*\*\* Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

*On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.*

*The editor will be glad to publish any queries consonant with the character of the journal.*

#### Low Temperatures.

IN your issue of Jan. 27, page 50, it is stated that the Franklin Search Expedition, under Lieutenant Frederick Schwatka, in 1879-80, experienced a temperature of  $-71^{\circ}$  C.

This is an error, as I have heard Lieutenant Schwatka in many conversations refer to it as "seventy-one degrees below zero, Fahrenheit."

I enclose a copy of a letter now in a collection belonging to my brother:—

TACOMA, WASH., Sept. 15, 1892.

On the third of January, 1880, my Arctic exploring party encountered a degree of cold of seventy-one below zero, Fahrenheit, or one hundred and three degrees below the freezing-point of that scale, the coldest we noted on the trip, and the coldest ever encountered by white men travelling in the field, for that day we moved our camp some twelve miles. It will be a cold day when that record gets left.

FREDERICK SCHWATKA.

FRED. G. PLUMMER.

Tacoma, Wash., Feb. 11., 1893.

#### Where is the Litre?

IT must be a source of regret to all interested in metrology that so much time was expended in the preparation, and so much space in the publication of the leading article in *Science* for March 17, entitled "Where is the Litre?" etc. Even if the instruction contained in the article be reinforced by the amusement which it furnishes, the result is quite incommensurate with the labor which must have been involved in its production.

Ignorance of the recognized principles of metrology has led to certain conclusions which will generally be harmless on account of the very magnitude of their errors. The sermonizing finish to the article, beginning with the sentence, "In spite of the much lauded simplicity of metric measures," etc., may, however, mis-

lead a few readers whose ideas have been befogged by the perusal of the previous three pages. It will be well to remind them, therefore, that the apparent bewildering confusion as to the value of the litre has no relation whatever to the "simplicity of the metric system." Indeed, the confusion might have been rendered vastly greater, the alleged case against the metric system much stronger, and the entire article more picturesque, if the author had introduced the "gus" of Arabia, the "pik" of Egypt, and the "sun" of Japan, the value of each of which in metres must always be a matter of considerable uncertainty.

The following simple statements may be of value. It is generally agreed among metrologists that *natural* standards of length and mass are not at present easily attainable. Our knowledge of physical or astronomical constants must continually increase in precision as methods and instruments are improved. Such constants are, therefore, unsuitable for standards, because standards should, first of all, be invariable as far as possible. Artificial standards can be made of more convenient dimensions, can be multiplied with almost any required degree of precision, and their invariability is perhaps as well assured as that of any suggestive national standard.

It was originally proposed to derive the metre from the dimensions of the earth. We know that the metre is not the one ten-millionth of the quadrant of the meridian passing through Paris, but that fact does not in the slightest degree lessen the value of the metre as a unit of length. Its value is so nearly that, that it is exceedingly convenient to use in ordinary calculations relating to the earth, not requiring a high degree of precision.

It was also proposed originally to establish some sort of a simple relation between the unit of length and the unit of mass. As length and mass have no natural relation to each other, any numerical ratio must depend on a physical constant, namely, the density of some selected kind of matter. The determination of this must be a matter of experiment, and its value can never be absolutely known. For this reason any relation between the unit of length and the unit of mass must always be an approximation. The unit of mass must, therefore, be an artificial, independent unit.

The new international prototype of the metre is, in length, an exact reproduction of the old metre of the archives, as far as can be determined by the most recent and most perfect means of comparison. The new international prototype kilogramme is identical, in mass, with the old kilogramme of the archives, as far as can be determined by the most precise and delicate weighings ever made.

It was originally intended that the mass of the kilogramme of the archives should be that of a cubic decimetre of pure water at its maximum density. As this involves the knowledge of a physical constant, it was not possible to realize this relation exactly, and it never will be possible.

In determining volumes which do not exceed a certain limit, it has been found that greater accuracy can ordinarily be secured by the indirect method of determining the mass of a liquid of known density, than by direct geometrical processes. The application of the latter requires simple forms whose linear dimensions may be easily and accurately measured. The former depends only on the accuracy attainable in mass measurement and density determination.

This method of volume measurement has usually been regarded, however, as a matter of convenience only. Thus, the U. S. gallon is defined as a volume of 231 cubic inches; in standardizing measures of capacity in gallons, it has always been customary to use the indirect mass-density method. The mass of water which has been assumed to represent this volume has varied from time to time as our knowledge of the physical constants involved advanced.

The litre was originally assumed to be identical in volume with the cubic decimetre, and there could be no possible objection to confining the term *litre* strictly to this meaning. But, as noted above, it being vastly more convenient to use the mass-density method in determining volumes, much of the uncertainty of precise volumetric work would be avoided by defining the litre as the volume of a kilogramme of water at maximum density.

Recognizing the wisdom of this course, the International Committee of Weights and Measures, in October, 1880, resolved that in its publications and in its official use the term *litre* should be used to express the volume of a kilogramme of pure water at maximum density. The one-thousandth part of this, that is to say, the volume of a gramme of pure water at maximum density is called the *millimetre*, and the abbreviation *ml.* is used to stand for it.

The *litre* and the *millilitre*, therefore, are not precisely identical with the cubic decimetre and the cubic centimetre. The difference, however, is very small, and may safely be neglected in all ordinary operations. Where a high degree of accuracy is required, it will usually be found that the results are primarily obtained by the mass-density method, and that no correction is required.

The International Bureau is engaged in an elaborate investigation of the relations of mass, volume, and density in pure water, and, when the results are available, they will doubtless satisfy the most exacting demands.

T. C. MENDENHALL.

Washington, D.C., April 14.

### On the Teaching of Biology.

IF the article "On the Emergence of a Sham Biology in America," by Mr. Conway MacMillan, printed in *Science* for April 7th, 1893, had appeared in a special journal, it would not be worth while to notice it, but since *Science* reaches many people who are not specialists in any branch of biology, it may not be a waste of time to point out some of its special merits.

The author of the article looks over the courses offered in biology in some of the leading universities of the country, and, finding that botany does not receive adequate treatment, he apparently becomes fired with the serious purpose of exposing what he illogically calls a "sham" science.

The Johns Hopkins University, which has done as much as, if not more than, any other single institution in the country, for the advancement of biological science in America, during the last seventeen years, is stigmatized in a way which will highly amuse those who are acquainted with its work. This institution is accused of dishonesty in naming its zoological courses. "Injustice," "wrecker-light use of the word 'biology,'" "protective mimicry in a university curriculum," "perpetrating a confidence game upon a board of trustees," are some of the choice phrases which are indulged in. These flattering remarks are not limited to the institution; they extend even to its graduates. "The cool effrontery of this would have surprised me had I not known the marvellous, sometimes continuous, sometimes sporadic, always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical."

Of course it is not necessary to take such criticism as this seriously. The tone of the article is so thoroughly bad, and the looseness of statement so completely inconsistent with anything bordering on scientific accuracy, that sober criticism is well nigh impossible.

The chief merit of the paper lies in pointing out the great value which a good course in general biology, such as that given for many years at the Johns Hopkins University, may possess for an average student, who will follow it with a fair degree of fidelity. Such a student would have learned what Lamarck, Treviranus, and Bichat comprehended, and what Huxley and the school of biologists who have been inspired by his teaching have striven with signal success to inculcate,—that the study of biology is not, as this erratic writer supposes, two disciplines, but *one discipline*, the study of living phenomena, in which the distinction between plant and animal, in the widest sense, is one of secondary importance.

A student who had followed this general biological course with a fair degree of success would have learned that "biological science is *not* to be set over against physical science in the broadest sense," but that in this broadest sense biology is a physical science, coördinate with chemistry and physics. In biology there is no natural cleavage into two branches, botany and zoölogy, any more than there is a natural constriction of chem-

istry into the studies of minerals and the compounds of carbon, because the plane of division in either case would be a purely imaginary one. An appreciation of this truth does not conflict with the obvious fact that biologists in general find it convenient to specialize either in the direction of the study of plants or the study of animals. Biology is often primarily divided, for convenience, into study of living structure and study of function, or into morphology and physiology, because the study of living structure is one subordinate discipline, and the study of function is another. For further convenience we may further classify these sub-sciences, according to their subject-matter, into vegetable morphology and animal morphology on the one hand, and into vegetable and animal physiology on the other.

Let an institution that sets about to teach biology do all it can to put before its students the principal facts of morphology and physiology of both plants and animals, but to pronounce its work, if well done, a "sham," through its inability to cover the whole field, is, to say the least, a very flagrant misuse of language. The title of Mr. MacMillan's article is misleading, and the whole tone of it is characterized by this glaring misuse of words. He does not distinguish between a "sham" science and a science too much "restricted" or "narrowed." Even if we grant the most that is said in regard to the teaching of biology at some of the institutions named, all that would be proved would be that the science of biology had been too much restricted at these places, not that there was any element of "sham" in it. The work which the Johns Hopkins University has done for the study of biology in this country proves conclusively that there has been no element of "sham" in its methods.

I find in the Johns Hopkins University Circulars for March, 1893, No. 104, eleven courses offered to students in the biological department, including seminaries and clubs. One course is announced in "Cryptogamic Botany"; the rest have reference almost exclusively to animal physiology and morphology. An elementary course in botany has been given at this university for years, and lecture courses in vegetable morphology and physiology of a more technical nature have been offered from time to time, showing that the study of plants is far from being ignored. The biological work of this university, as is well known, has been chiefly devoted to the study of animal physiology and morphology, and the work that it has undertaken it has done eminently well. Nothing could be more unjust than any inference that this university has encouraged its students to undervalue the study of plants. On the contrary, it has regretted that it has had no fully equipped botanical laboratory to offer its students, and it has uniformly advised them to go to institutions better equipped in this department for the special study of plants.

It is not possible for every institution to take the same color with reference to the special lines of scientific investigation, but this is a different thing from saying that it is not desirable for every institution to have a well-balanced curriculum. In most of the smaller colleges the man at the head of his department is the only teacher in it, and if he is a botanist his work will soon take on a botanical tinge; if morphologist or physiologist, his special work is sure to come to the front. This explains a good deal of the "sham" element that Mr. MacMillan has discovered in American biological teaching.

The stimulus which comes from the association of specialists in a large educational centre is undoubtedly very helpful, but as soon as students commence to leave the elementary stages of their work, and to enter upon special lines of investigation, their sympathies immediately diverge with increasing rapidity. It is therefore desirable that this loss of sympathy on the part of one specialist for the work of another, should be postponed as long as possible. One means of accomplishing this in a large university, in the case of biology for instance, is undoubtedly to present the whole subject in the fullest manner, especially in the elementary courses.

There is no doubt that every biologist, whatever the special line of work to which he devotes himself, should have the same training up to the point of specialization, in at least chemistry, physics, morphology, and physiology. The attitude of mind which Mr. MacMillan displays comes from a lack of this early compre-

hensive training, or at least from the lack of profiting by it. If while himself a student at the John Hopkins University, he had determined to get all there was in the admirable elementary courses which are there offered in general biography, zoölogy, animal physiology and embryology, instead of interesting himself from the first mainly in plants, he would not only have been enabled to take a broader view of his specialty, but would not have committed himself to the position in which this article places him.

Mr. MacMillan incidentally remarks that he has "not at present time to discuss the fundamental absurdity of courses in 'general biology,' as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves. It is as if one should enter upon analytical statics and follow it up by geometry and the calculus." Here again Mr. MacMillan demonstrates the urgent need of a good course in general biology for botanists as well as for zoölogists. Here the analogy drawn is false. Zoölogy and botany do not bear a similar relation to biology that geometry and the calculus bear to the higher mathematics. The instruments for solving problems in botany and zoölogy are essentially the same, such as good observation, sound reasoning, a knowledge of technical methods, and of the other physical sciences.

It is not necessary for the student to examine a large number of organisms in order to come face to face with the fundamental properties of living things, and this fact proves that Huxley and his successors are right in insisting that the study of biology is one discipline. To teach the student this, and to lead him to discover some of the wider agreements and differences of living organisms, is of more intellectual value to him than to conduct him at the start to the more special study of either plants or animals. This is true whether he is to become a specialist in biology or not.

Some of the chief merits of Mr. MacMillan's paper have now been pointed out. A subordinate merit which it possesses is that of calling attention to the defect in many institutions of not including botany in their curriculum, or in not giving it the prominence which it deserves. If he had limited himself to pointing out this defect, without casting slurs upon honored institutions and their graduates, in an offensive way, his article might have done good.

FRANCIS H. HERRICK.

Adelbert College, Cleveland, Ohio, April 15th, 1893.

#### A New Source of the So-Called Mexican Onyx.

LOVERS of the beautiful, in the way of high-grade material for decorative work, will be pleased to learn of the recent discovery, on the peninsula of Lower California, of extensive deposits of the so-called Mexican onyx. The new find is some 150 miles south-east of San Diego, and 50 miles from the Pacific coast. The material, as is the case with that of Mexico proper and other sources, is a travertine (i.e., a spring deposit) and not stalagmitic. The deposits are essentially superficial, the material in many instances so occurring as to be taken directly from the surface of the ground by means of bars and without previous stripping. The colors are light green, rose, and white, variously veined and tinted, and of great beauty, while in compactness of texture, susceptibility to polish and freedom from flaws, the material leaves little to be desired. A company has already been organized for working the deposits, and the first shipment has reached St. Louis, to be cut and polished for exhibition at Chicago during the World's Columbian Exhibition.

GEORGE P. MERRILL.

Washington, D.C.

#### BOOK-REVIEWS.

*The Metaspermæ of the Minnesota Valley.* A list of the higher seed-producing plants indigenous to the drainage basin of the Minnesota River. By CONWAY MACMILLAN. Minneapolis, 1892. 839 pp. 2 Maps. 8°.

BOTANISTS will examine this volume with interest, because of the numerous new features it presents. It is the first of the botanical

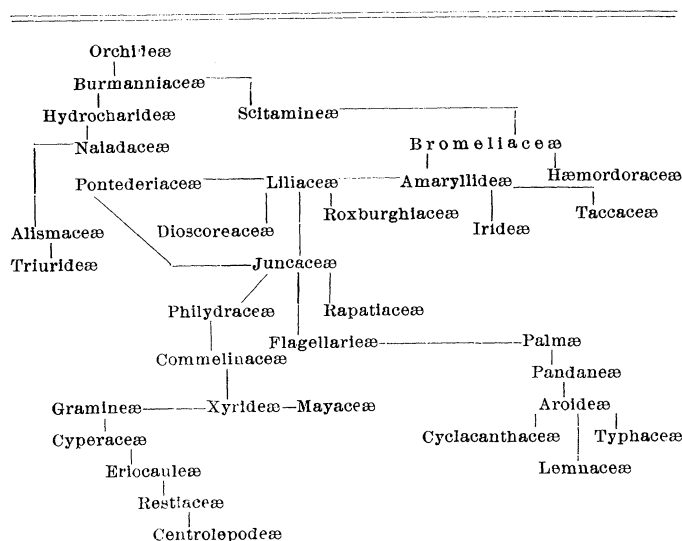
reports of the Geological and Natural History Survey of Minnesota, and, while entirely local in its character, it is very far beyond the usual local catalogue. It contains a record of 1,174 species and varieties, distributed among 407 genera and 106 families. Under each family reference is made to the place of its original characterization, the number of genera and species, living or extinct, it contains, and its distribution in a very general way. Under each genus we have the synonymy as fully as may be, again with a reference to the number of species and their more detailed distribution. Finally, under each species and variety the synonymy is given, still more detailed distribution, and mention of herbaria where specimens are to be found. It will thus be seen that, while it is a catalogue of plants, it is one in a wider sense than the majority of such publications. Its interest and value to botanists lie not alone in the various facts above referred to, but because it discards the time-honored arrangement of orders, such as is found in the ordinary manuals and text-books, and introduces the newer and more natural system of classification. It contains, besides, a discussion of the factors upon which classification is based, principles of geographical distribution, and extraordinary statistical detail respecting the plants named in the list.

We turn first to the classification and nomenclature. We well recollect when we first began to study botany, the feeling of satisfaction that was felt at the seeming stability of the science. We had been familiar with the discussions of zoölogists and geologists regarding the condition of nomenclature in their respective branches, and the botanical manuals gave no sign of changes that were to come, or indicated the presence of dangerous ground. But rumblings of the coming eruption were soon heard, although it was not until the publication of that amazing book of Kuntze's, "Revisio genera plantarum," which has turned everything upside down and set the whole botanical world by the ears, that the full violence of the eruption was realized. Against many of the suggestions of this reformer there has been open revolt, but upon the whole the effect has been good. It is true it has compelled those who learned their botany some years ago to learn much of it over again, and has made our latest text-books obsolete or old-fashioned, but it has also put the science upon a more stable foundation.

The discussion of generic and specific names has introduced the perennially fertile subject, a natural classification of orders. The plan of placing Ranunculaceæ at the head of Anthophyta and Gramineæ at the foot is so familiar that scarcely any other seems possible. It has been recognized, however, that the system was very faulty, and numerous endeavors have been made to change it. As long ago as 1833 the present writer, in an article entitled "On the Position of the Compositæ and Orchideæ in the Natural System,"<sup>1</sup> pointed out that the old arrangement was far from being the best; and he made some suggestions as to what families should take the highest rank. He suggested that among dicotyledons Compositæ should be regarded as the highest, inasmuch as here is found the largest production of seed (the end of all plant life) with the least expenditure of material, and, at the same time, with ample provision for cross fertilization. The immense number of species and their great range were also cited to prove their high position. The impossibility of arranging the orders in a strictly natural and yet lineal system was recognized, but it was suggested that the Labiatæ were somewhat parallel with the Compositæ in their differentiation; while with that order were associated, as near allies, Verbenaceæ, Boragineæ, and Scrophularinæ. Among polypetalous orders Leguminosæ was placed highest, followed closely by Rosaceæ, Saxifragaceæ, Umbelliferae, and Ranunculaceæ. Among monocotyledons the Orchideæ were accorded the highest rank, mainly because of their large numbers, wide distribution, varied form, and elaborate means for cross fertilization. At the same time, a general scheme was proposed, which is reproduced here. In it, it will be observed that there are four general lines of descent, viz., from Orchideæ, Liliaceæ, Palmæ, and Gramineæ. The relative rank of the smaller orders is not that which has been followed in the volume under review, but the

<sup>1</sup> American Naturalist, December, 1883. Also read in the Minneapolis meeting of the A. A. S.

Chart of the Monocotyledons.



placing of Orchidaceae at the head of one alliance, and Gramineae of another, agrees with the general scheme. Again, among the dicotyledons Compositae is regarded as the highest and Ranunculaceae is placed well down in the scale. The whole arrangement is that of Engler and Prantl,<sup>2</sup> but it corresponds so well with the provisional outline suggested by the writer of this that he has called attention to it.

Professor MacMillan's plan in the citation of authorities is in

<sup>2</sup> Natur. Pflanzenfam., 1887-1893.

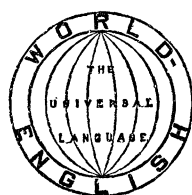
#### CALENDAR OF SOCIETIES.

##### Anthropological Society, Washington.

April 18.—J. Owen Dorsey, Siouan Photogenic Types; James Mooney, The Indian Messiah and the Ghost Dance (illustrated by Lantern Slides); Henry Gannett, Estimates of Wealth.

##### Society of Natural History, Boston.

April 19.—J. B. Woodworth, Traces of a Fauna in the Cambridge Slates; Charles P. Bowditch, Ruins of Central America.



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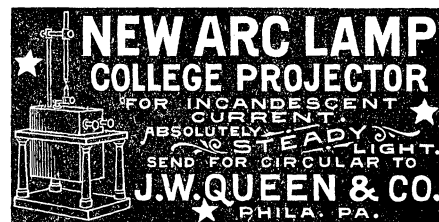
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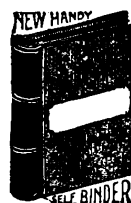
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which equals the old group Gamopetalæ. The classification may be tabulated as follows:—

- A.—Protophyta.
- B.—Metaphyta.
  - I. Gamophyta.
  - II. Sporophyta.
    - (1) Thallophyta.
    - (2) Archegoniata.
    - (3) Metaspermæ.
      - a. Chalazagamæ (a single genus *Casuarina*).
      - b. Porogamæ.
        - \* Monocotyledons.
        - \*\* Dicotyledons.
        - † Archichlamydeæ.
        - †† Metachlamydeæ.

While following the classification of Engler and Prantl to a large extent, Professor MacMillan takes issue with them upon some points. One of these is the nature of the Mycetozoa, or, as they are more commonly called, Myxomycetes. He believes them to be animals rather than plants. It is difficult for the present writer to see why the motility of the plasmodium of these plants, really the only animal-like feature they possess, should be regarded as very different in character from the motility of the swarm-spores of such universally recognized plants as *Hydrodictyon*, *Cedogonium*, *Volvox*, and others. If motility in conjunction with lack of chlorophyll be evidence of the animal nature of the Myxomycetes, why should not the stationary *Hydra*, multiplying (as it can) when cut into small pieces, and possessing chlorophyll, be considered a plant? Or where are we to place *Dionæa*, *Drosera*, and the like, that present movements analogous to, if not precisely the same as, the contractile powers of many animals? The hard and fast lines between the animal and the vegetable kingdoms have long since been broken down, and there is as much justice in placing Myxomycetes with vegetables as in placing Protista with animals. The fact of motility, the absence of chlorophyll, or both combined, can scarcely be sufficient to

overbalance the preponderance of facts showing the distinct vegetable nature of the slime-molds.

There is a long and interesting discussion of the relationships of the metaspermic flora of the region, in which the general features of geographical distribution and the factors concerned with it are considered. The history of the region is regarded as of prime importance, and past time has been a most potent factor in the work. As the Metaspermæ have existed on the earth since as far back as Jurassic times, this history begins then. Glacial time, however, was the period most immediately concerned with the present distribution. Many plants previous to that epoch lived in Minnesota which were driven away never to return, or which were entirely exterminated; while, on the other hand, many species were found after the close of the period entirely unknown before. The Sequoias, once widely spread over the continent, but now occupying so restricted a range, he considers to be an indication of the great competition existing between plants in Tertiary times, their great height and giant bulk showing the magnitude of the struggle. So, on the other hand, the Compositæ are regarded as representing a type that permitted wonderful variation, and hence great adaptability to changed conditions.

The 150 pages devoted to statistics of the flora can only be mentioned here. Even a detailed statement of the heads treated of would give but a faint idea of the elaborateness of the discussion. A very full index (66 pages) gives easy reference to all species, genera, and orders mentioned. Altogether this is the most elaborate catalogue of plants of a limited district it has ever been our lot to examine. Its use of modern classification and nomenclature will make it of very great interest and value to all systematic botanists, and, while all may not agree with the author in his many suggestions or innovations, there are none but will recognize the enormous amount of labor put into the volume. It is greatly to be desired that other limited floras be as completely dissected and discussed.

JOSEPH F. JAMES.

Washington, D.C., March 27.

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For sale or exchange.—I have a Caligraph typewriter (No. 2) in perfect order and nearly new. It is in a heavy leather, plush-lined office case, the whole costing me about \$100. I desire to obtain for it, either by sale or exchange, a new No. 5 "Kodak" camera, with six double feather-weight plate-holders and the latest pattern of their tripod. The lens and pneumatic time-shutter must also be the same as those now sold with the last No. 5 Kodak. The price of what I desire in exchange is \$78. Address, for particulars, P. O. Box 314, Takoma, District of Columbia.

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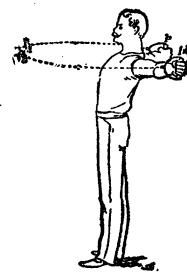
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